R.J. Barro (ed.), *Modern Business Cycle Theory*, Basil Blackwell, Oxford, 1989. Pp. 337. £ 32.50

Conventional macroeconomic theory has proceeded for a long time along lines that business cycles and long-run growth paths are two entirely different phenomena. Business cycle theory referred to stationary stochastic time paths, whereas growth theory was concerned with the long-run development of economies. With Keynes' sneer about the futility of long-run analysis given the brevity of life, it was thought that growth theory was well suited for the ivory tower economist. The eighties have seen the emergence of a second generation New Classical macroeconomists, who tried to repair this unfortunate separation. Fluctuations in economic activity are to be understood as the reflection of shocks in endowments, technology, preferences, the dispersion of information and the rules of the game. Starting with Kydland and Prescott's [1982] simple business cycle model, a whole new field of research has appeared, which Barro sums up in *Modern Business Cycle Theory*. It presents a snapshot of the state-of-the-art macroeconomic theory. The first half of the book is set in a purely competitive world, in which intertemporal choices are made without the interference of government. The second half does pay attention to the intricacies of economic policy design.

Bennett McCallum opens the volume by reviewing real business cycle models, *i.e.* general equilibrium models that stress the spreading over time of the effects of non-monetary shocks. As an introduction he demonstrates how technology shocks can be the driving force behind real business cycles (RBC). The ultimate objective of RBC models is to explain economic fluctuations quantitatively. Although McCallum examines the quantitative aspects of RBC models, he does not completely clarify the 'fuzzy' econometrics, as often used in such models. I miss here a formal probability model for which the RBC procedures can be shown to produce estimators with desirable statistical properties.

A second objection one can make to present-day RBC models is the preoccupation with technology shocks. The state of technology and its rate of change are often used in explaining business cycles and differences in production and consumption levels across countries. It is, however, at the same time one of the most puzzling phenomena which unfortunately must still be called the 'measurement of our ignorance.' To explain business cycles by referring to serially correlated 'ignorance' is a weak shot. Prescott [1986] acknowledges that business cycle theory is far ahead of measurement, but I fail to see why additional, better measurable variables are not used as explanatory variables. Van Imhoff [1989, chapter 4.2] has shown that a once-and-for-all shock in population growth can give rise to persistent real business cycles. The conspicuous absence of this particular shock from most business cycle theories becomes more remarkable since Barro has recently collaborated with Gary Becker [*e.g.* 1989] on the theory of endogenous fertility choice.

Paul Romer (chapter 2) gives an overview of the role of capital accumulation in the theory of economic growth. Capital refers to physical as well as human capital. Romer starts from scratch by using a set of stylised facts of economic growth to be explained by the production factor capital. In an elegant manner he uses the Kuhn-Tucker theorem to characterise a variety of dynamic competitive economies. New additions to growth theory such as externalities of knowledge, imperfect competition, increasing returns and endogenous population growth are briefly discussed.

Sanford Grossman (chapter 3) gives a short introduction to the use of rational expec-

tations in situations where information is dispersed. It is a good reminder for the reader who strongly believes in the merits of the market that he or she is skating on thin ice when the assumption 'there are no costs in obtaining information' is dropped. Robert Hall (chapter 4) presents an overview of the work that has arisen out of Lucas' [1976] thesis that 'there is no such thing as a consumption function' (thereby referring to the econometric practice to assume a stable relationship between permanent income and consumption). Hall tries to explain only one 'fact': the rate of change of consumption can be predicted by past values of real income and past values of financial variables. Two factors – durability of consumption goods and liquidity constraints – seem to explain this stylised fact well.

The second half of the book begins with a chapter by Robert Barro, who reviews the macroeconomic effects of government expenditures, distortionary taxation and public debt. Barro mixes, in a provocative manner, theory with empirical evidence. Especially noteworthy is his discussion of the assumptions of the Ricardian Equivalence theorem. In the next three chapters Barro categorizes strategic questions of fiscal policy. Kenneth Rogoff (chapter 6) discusses how the reputation of a government can substitute for legal constraints on macroeconomic policy. He applies this question to the specific problem of monetary policy. Chari, Kehoe and Prescott (chapter 7) apply the problem of time-consistency to questions of capital taxation and public debt default. The novel aspect of these theories is the use of game theory in modelling economic-political institutions, thereby remembering a lesson from the field of politico-economic theory that institutions matter (see Mueller [1987]). The last chapter by Neil Wallace (chapter 8) discusses different types of monetary models and their implications for the portfolio strategy of a government.

Reviewing the contributions, one can draw a few conclusions from the lessons in shocks. First and foremost, macroeconomics is not an independent subject anymore. The various fields of economics – finance theory, industrial organisation, microeconomics, international trade and public finance – are all merged with the ultimate objective of explaining *the* business cycle. At the same time one can notice that the distinction between post-, neo- (or whatever) Keynesian, Monetarist and New Classical economics becomes rather vague and is perhaps an obsolete division of schools of thought. The only remaining distinction is one of equilibrium and non-equilibrium analysis. These observations are not novel (see *e.g.* Lucas [1987]) but they are becoming increasingly clear with every step taken by business cycle theorists.

Secondly, I must say that one of the inherent drawbacks of reviewing developments of such recent date are the numerous loose ends, doubtful modelling strategies and ambiguous dynamics. The models are often of a rudimentary nature and any policy conclusion deduced from these models should be subject to the Golden Rule of Policy Design: any theory or empirical finding should ferment for at least five years before it is translated into policy. Although Prescott [1986] is rather pleased and perhaps satisfied with the explanatory power of real business cycle models. I think there are too many missing pieces of the business cycle puzzle to be content with the state of the art. International trade and factor flows, endogenous fertility and financial intermediation are amongst the most prominent missing pieces.

One of the last points that should be mentioned concerns the testing of business cycle models. Kydland and Prescott [1982] restrict the number of free parameters by a number of steady-state restrictions and by the extensive use of out-of-sample parameter estimates. Simulating time paths, variances and covariances of key economic variables

with this methodology seems to work remarkably well, but how can we be sure that the calibrated model truly reflects structural relationships? For instance, in Prescott [1986] technology shocks are the only driving force behind cyclical fluctuations. However, any microeconomic evidence is lacking on technology shocks. Besides, if one really wants to simulate periods other than the post-World War II period, more research is needed on *e.g.* the relationship between technology shocks and depreciation or the relationship between fertility and time preference.

A much neglected aspect of down-to-earth-economics is the art of choosing models in describing actual economies. Given the complex dynamics of rational expectation models (especially with the introduction of game theory) the chances are that the ordinary economist will loose track and fall back on the (sometimes misleading) static Marshallian cross of demand and supply. To a large extent, Barro *et al.* have set the record straight by offering an accessible, frank and exciting overview of modern business cycle theory.

Hendrik P. van Dalen

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S. Solomou, *Phases of Economic Growth, 1850-1973*, Cambridge University Press, Cambridge, *etc.*, 1988. Pp. xvi + 197.

The observation that economic development obviously does not occur smoothly but rather in 'cycles' or 'waves' of different lengths and intensities, has fascinated economists since the last century. The interest in these phenomena seems to have increased in the last few years. Quite a lot of papers and books dedicated to 'long wave research' have been published. Before entering into a discussion on the recently published works of S. Solomou (Phasis of Economic Growth, 1850-1973, Cambridge, 1988) and J. Reijnders (The Enigma of Long Waves, Groningen, 1988) I would like to mark my own position in the discussion of 'long waves': I do not say that there is something like